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DOI:

[10.1016/j.ecolecon.2017.12.015](https://doi.org/10.1016/j.ecolecon.2017.12.015)

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*Document Version*

Peer reviewed version

*Citation for published version (Harvard):*

Beltran Hernandez, A, Maddison, D & Elliott, R 2018, 'Is flood risk capitalised into property values?', *Ecological Economics*, vol. 146, pp. 668-685. <https://doi.org/10.1016/j.ecolecon.2017.12.015>

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# Is Flood Risk Capitalised into Property Values?

*Allan Beltrán<sup>†</sup>, David Maddison\*, Robert Elliott\*\**

## Abstract

Economic theory suggests that, other things being equal, properties located within a floodplain should suffer a price discount. A survey of the existing evidence nonetheless reveals that this price discount lies anywhere between  $-75.5$  percent to a  $+61.0$  percent price premium. The objective of this paper is to summarise and explore the wide variation in the empirical results to obtain ‘best’ estimates with which to guide policy decisions. Results from our meta-analysis comprising 37 published works and 364 point estimates indicate the existence of marked differences between studies according to when and where they were conducted. For coastal regions the results show that properties located in the floodplain command higher prices; this finding is however likely to be caused by a high correlation between omitted coastal amenities and flood risk. There is moreover, some evidence that publication bias affects the coastal flooding literature. Results from meta-regression analyses intended to uncover sources of heterogeneity confirm that controlling for time elapsed since the most recent flood is especially important. For inland flooding the price discount associated with location in the 100-year floodplain is  $-4.6$  percent. Although other estimates are defensible, we suggest this figure be used as a rule of thumb to determine the benefits of flood relief projects to households.

**Keywords:** Flood Risk, Hedonic Valuation, Meta-Analysis, Meta-Regression

**JEL Code:** Q51, R21, Q54

Acknowledgements: We thank conference and seminar participants at the 2015 EAERE Annual Conference (Helsinki, Finland), the 2014 Economic Research Network Meta-Analysis Colloquium (Athens, Greece), and the 2014 EAERE-FEEM-VIU Economics of Adaptation to Climate Change Summer School (Venice, Italy) for helpful comments. Allan Beltrán thanks support from CONACYT (National Council of Science and Technology-Mexico) and the University of Birmingham for a graduate scholarship. We are grateful to three anonymous referees for comments made on an earlier version of this paper.

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# Is Flood Risk Capitalised in Property Values?

## 1. Introduction

Since the year 2000, there have been over 2,800 major flood events reported globally (EM-DAT, 2017).<sup>1</sup> The economic losses reported to have been caused by these floods exceeds 538 billion USD globally (EM-DAT, 2017). Asia has been the region most affected by flooding in terms of the number of flood events (40 percent), number of victims (70 percent) and total flood damages (59 percent). Other regions such as Africa and the Americas have also been badly hit with each of these regions accounting for about 20 percent of the total number of flood events. Although the number of flood events that occurred in Europe represents only 13 percent of the total the economic losses in this region over the last 15 years account for 22 percent of global damages (EM-DAT, 2017). By the year 2050 the annual expected losses from floods are predicted to exceed 1 trillion USD (Hallegatte et al., 2013; World Bank 2013; Munich Re, 2013).

Because of the scale of the problem, combatting floods is a significant policy issue. Considerable sums continue to be earmarked for flood relief projects although it is widely accepted that it is neither desirable nor feasible to protect all localities from flooding. Cost-benefit analysis of such projects entails comparing the monetised costs and benefits of each alternative and determining which yield an acceptable cost-benefit ratio. This enables scarce resources to be efficiently allocated between flood prevention schemes and other public works. In such analyses however

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<sup>1</sup> As reported by “EM-DAT: The Emergency Events Database”. To consult the inclusion criteria for quantification of natural disasters in the EM-DAT database see: <http://www.emdat.be/>.

the main challenge is to provide accurate measures of the benefits, not least because these combine both market and nonmarket impacts.

Some studies quantify the economic benefits of structural flood protection measures using a damage cost approach (Brouwer and Van Ek, 2004; Blonn et al., 2010; Jongman et al., 2012). Others use stated preference valuation techniques to estimate willingness to pay (WTP) for flood protection (Brouwer et al., 2009; Phillips, 2011; Veronesi et al., 2014). There are also studies estimating the economic value of flood protection afforded by natural ecosystems (Bateman and Langford, 1997; Bateman et al., 2001; Gibbons et al., 2014) and other non-structural defence measures (Holway and Burby, 1990; Meyer et al., 2012; Troy and Romm, 2004). Kazmierczak and Bichard (2010), Bramley and Bowker (2002) and Osberghaus (2015) consider the determinants of private flood mitigation measures. Another popular approach is to use hedonic analysis to estimate the benefits of a reduction in flood risk.

In an efficient housing market the price of property located inside the floodplain ought to be lower than the price of equivalent property outside. This price discount is interpreted as a measure of the benefits of a reduction in flood risk. Numerous authors have investigated the effect of location in a 500-year or 100-year floodplain on property prices for both inland and coastal locations. The results are however inconsistent and sometimes point to the presence of a price premium rather than the expected discount. Without a meta-analysis it is difficult to suggest a 'best' estimate of the percentage discount for floodplain location.

The use of meta-analysis is ubiquitous in environmental risk analysis. Smith and Huang (1995) use meta-analysis to infer WTP for reductions in air pollution based on evidence from hedonic studies. Nelson (2004) also conducts a meta-analysis of hedonic estimates of WTP for a reduction of noise from airports. More recently, Hjerpe et al. (2015) estimate the value of ecosystem conservation by means of a meta-analysis combining different stated preference valuation studies whilst Bergeijk and Lazzaroni (2015) use a meta-analysis to analyse the macroeconomic impact of natural disasters. Siriwardena et al. (2016) estimate the value of tree cover using a meta-analysis of hedonic studies undertaken in the US.

We make four main contributions to the literature. First, we update the only other meta-analysis of the discount for floodplain location. In so doing so we double the number of research papers and treble the number of observations used. Second, our analysis changes the way that primary studies contributing more than one study are weighted. Third, we conduct tests for publication bias and, finding that studies of coastal locations are severely affected by publication bias, exclude them from further analysis. Finally and most importantly, we collect supplementary information enabling us to control for the recent flood-history of locations where studies were undertaken.

The only existing meta-analysis of flood risk is Daniel et al. (2009a) who employ a meta-sample comprising 19 empirical studies and 117 estimates. They use meta-regression to explore the variation encountered using a set of 18 explanatory variables describing the spatio-temporal features of the studies, the design characteristics and the controls included in the original studies.

Their paper finds that an increase in the yearly risk of flooding of 0.01 results in a change in house prices of  $-0.6$  percent.

We by contrast argue that knowing the flood history of areas in which primary studies were undertaken is of fundamental importance. The meta-analysis of Daniel et al. (2009a) distinguishes between those studies undertaken in periods during which there were no floods as well as difference in difference (DID) studies that provide separate estimates of the effect of floodplain location both before and after a flood event. They ignore the fact that all study locations possess a prior flood history the consequences of which may still be present. Distinguishing between studies by adding dummy variables identifying a before-the-flood or an after-the-flood DID estimate does not adequately control for recovery in prices. Our paper uses meta-regression to control for time elapsed since the most recent flood event in all of the primary studies.

Judging by the information we have assembled, existing studies tend to be undertaken in areas hit by recent floods rather than in areas which, although located in the floodplain, have avoided recent flooding. But by focusing on such sites the floodplain discount might have been overestimated.

Once we eliminate studies dealing with coastal flooding and control for the time elapsed since previous flood events, our preferred price discount for location within a 100-year floodplain is almost an order of magnitude different to that of Daniel et al. (2009a). Our findings call into question the simple pooling of studies undertaken in locations with different flood histories.

They also serve as an example of how including in meta-analyses a set of observations that are severely affected by various forms of bias can seriously alter findings.

The remainder of our paper is arranged as follows. Section 2 describes the theory used to infer the impact of floodplain location, carefully distinguishing between the different sorts of evidence. Section 3 describes the data. Section 4 provides a meta-analysis of the change in property prices encountered in 100-year and 500-year floodplain locations. Section 5 addresses the issue of publication bias. Section 6 attempts to explain sources of heterogeneity in published results using meta-regression. Section 7 discusses these findings, assesses their robustness and explains why they differ from earlier ones. Section 8 concludes.

## 2. The Theoretical Model

This section defines the hedonic price function (HPF) in such a way as to consider explicitly the flood risk associated with particular properties. Based on the model of Hallstrom and Smith (2005), the subjective probability  $p$  that a property will be flooded is a function  $p(i, r)$  of the information set  $i$  that the individual possesses about flood risk in the vicinity, and  $r$  that represents all of the site attributes related to the risk of flooding e.g. elevation or proximity to water bodies. It is vital to differentiate the *subjective* assessment of the probability the house will be flooded from  $\pi$  which is the *objective* probability of flooding (Knuth et al. 2014). Nevertheless, in areas where the disclosure of the existence of flood risk is mandatory or publicly available, the set of information,  $i$ , might include the objective probability of flooding,  $\pi$ . The HPF is represented by the equation:

$$P = P(Z, r, p(i, r)) \quad (1)$$

Here  $P$  denotes the price of the house and, whilst this is exogenous to prospective buyers and sellers, depends on the subjective risk perception  $p(i, r)$ ;  $Z$  represents an additional set of structural, environmental and locational characteristics of the house not indicative of flood risk. Following Brookshire et al. (1985) the decision of the household is modeled using a state dependent expected utility ( $EU$ ) function:

$$EU = p(i, r) \cdot U^F[Z, r, Q] + (1 - p(i, r)) \cdot U^{NF}[Z, r, Q] \quad (2)$$

$U^F(\cdot)$  is the homeowner's utility in a state in which a flood occurs and  $U^{NF}(\cdot)$  is the homeowner's utility when no flood occurs. The variable  $Q$  denotes a composite commodity. The household's budget constraint is given by equation (3) where  $M$  represents income:

$$M = P(Z, r, p(i, r)) + Q \quad (3)$$

Maximising expected utility with respect to  $p$  subject to the budget constraint and then dividing through by the expected marginal utility of income results in the following expression:

$$\frac{\partial P}{\partial p} = \frac{U^F - U^{NF}}{p(i, r) \frac{\partial U^F}{\partial Q} + (1 - p(i, r)) \frac{\partial U^{NF}}{\partial Q}} \quad (4)$$

Equation (4) gives the coefficient on the subjective risk variable in the HPF. For optimality the implicit price of flood risk is equal to the difference in utility across states divided by the expected marginal utility of household income. Hence the household's locational decision provides a measure WTP for a marginal change in the probability of flooding.



Empirical applications of the hedonic technique can be divided into two sorts. The first sort is able only to identify the impact on prices of changes in the subjective probability of flooding. The second sort is able to identify both the impact on prices of changes in the subjective probability of flooding as well as the impact on prices of the changes in information affecting the subjective probability of flooding. And whilst there are other sources of information, the variable that most obviously impacts the subjective probability of a flood is the actual occurrence of a flood. Consider the following HPF:

$$\ln P_{it} = \alpha_0 + \sum_{j=1} \alpha_j Z_{ij} + \beta r_i + \gamma FPD_i + \delta Flood_{it} + \theta(Flood_{it} \times FPD_i) + \varepsilon_{it} \quad (5)$$

Here,  $\ln P_i$  indicates the log of the sale price of house  $i$   $Flood$  is a dummy variable that assumes the value unity if the transaction of property  $i$  happened after some flood event and  $FPD$  is a dummy variable which assumes the value unity if the property is located within the floodplain and  $\varepsilon$  is a property specific error which is assumed  $\varepsilon_i \sim N(0, \sigma^2 I)$ . Floodplain location here serves as a proxy for the subjective probability of flooding whereas the occurrence of the flood event potentially alters the subjective probability of flooding. The parameter  $\gamma$  measures the pre-flood relative price differential for house located in the floodplain,  $\delta$  measures the relative sale price differential for all those properties which were sold after the flood whilst  $\theta$  measures the impact on prices located within the floodplain arising out of the information conveyed by the flood.

Specifications like the one in equation (5) are termed DID models because they contain information about houses within and outside the floodplain, and a treatment i.e. flooding that effects only those houses in one group. Note also that according to the HPF the post-flooding price differential for houses located inside the floodplain is given by the sum of  $\gamma$  and  $\theta$ .

There is evidence that the information effect  $\theta$  will differ according to how long after the event it is measured. In particular, recent studies such as Atreya et al. (2013) and Bin and Landry (2013) find evidence suggesting that the information effect of flood events dies away. This suggests rewriting the HPF in such a way that  $\theta$  is time dependent:

$$\ln P_{it} = \alpha_0 + \sum_{j=1} \alpha_j Z_{ij} + \beta r_i + \gamma FPD_i + \delta Flood_{it} + \theta_t(Flood_{it} \times FPD_i) + \varepsilon_{it} \quad (6)$$

In this HPF the post-flood price differential is now  $\gamma + \theta_t$ . However, having admitted this, it is now no longer obvious whether the pre-flood estimate of the coefficient  $\gamma$  might itself not also be affected by earlier floods.

In reality all estimates of the floodplain discount are post-flood estimates; even if no flood occurred during the period under observation it might be that a flood occurred just prior to the start of the study. In addition, the same issue effecting pre-flood estimates from DID models will also effect estimates of the floodplain discount from hedonic models estimated over periods during which no significant flood event occurred.

Differences in estimates of the floodplain price discount depend on floodplain designation. Clearly a greater price discount is expected for location in a 100-year floodplain than in a 500-year floodplain. We are for the purposes of cost-benefit analysis interested in how the discount varies according to floodplain designation. However, heterogeneity in the floodplain price discount is caused by combining analyses with different flood histories. This heterogeneity

depends on the extent to which people overreact to flood events and how quickly the memory of such events fades. In order to control for this source of heterogeneity one has to know something about the flood history of each location.

Although it could certainly be the case that a flood event causes households to correct a poor subjective prior there are reasons to believe that the use of post-flood estimates from DID hedonic models is likely to result in biased estimates of the implicit price of flood risk in the housing market. The first argument is derived from Tversky and Kahneman's (1973) idea of an availability heuristic in the perception of risk; basically individuals assess the probability of an event by the ease with which the actual occurrence of such an event can be brought to mind (Wachinger et al. 2013; Knuth et al. 2014; Kellens et al., 2013). Cameron and Shah (2015) provide an illustration of this in a study of Indonesian households where they conclude that after a flood individuals inaccurately updated their perception of flood risk. In particular they find that individuals affected by flooding reported unrealistically high probabilities that another flood event would occur in the next year and that it would be severe, with an effect lasting for several years. The second reason for fearing bias is that studies looking at the post-flood discount of prices attribute the update in prices entirely to the effect of new information conveyed by the flood. They do not generally acknowledge the extent to which these changes might be driven by actual flood damages and a recovery of prices as individuals restore their property to its former condition. Atreya and Ferreira (2015) suggest that the majority of this effect might indeed be driven by flood damages, and that after identifying those properties directly affected by flooding there is no significant price update on other properties located inside the floodplain.<sup>2</sup>

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<sup>2</sup> A referee has pointed out to us a further issue: whereas a standard hedonic study can identify the implicit price for flood risk reductions, a DID identifies capitalisation in the housing market which is not directly interpretable as marginal WTP (Kuminoff

### 3. The Data

We follow the reporting guidelines for meta-regression analysis in economics provided by Stanley et al. (2013), who list the four basic steps necessary to undertake a meta-analysis. First, it is necessary to define the theoretical relationship of interest. Second, one has to collect the population of studies that provide data on the relationship of interest. The third step involves coding the studies and computing the effect sizes. We detail these two steps in this section. The final steps which we present in sections 5 and 6 are examination of the distribution of effect sizes and the influence of the moderating variables.

Our meta-analysis is founded on a systematic literature review employing widely-used proprietary databases and a secondary search through the bibliography of any studies thereby traced. Using Boolean operators the search was undertaken in English only. The combination of words used for this exercise were:

(Flood\* OR Inundat\* OR Hurricane\*) AND (Propert\* OR Hous\* OR Resident\* OR “Real Estate”)

The use of \* as the wildcard character permits us to enlarge the search by substituting a sequence of letters. For example, searching for *flood\** will retrieve, *flood*, *floods*, *floodplain*, *flooding* and so on.

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and Pope, 2014). For those who are concerned about this issue we also present results for hedonic and DID studies separately in appendix A2.

A number of databases e.g. the Social Science Research Network (SSRN) do not permit the use of wildcards or Boolean operators and here different strategies were employed. Other databases e.g. ProQuest retrieved too many irrelevant works. A chronologically-ordered summary of the studies identified in the literature review is contained in Table 1. This exercise successfully retrieved all of the papers in Daniel et al. (2009a) and many more besides.

**Table 1. Chronologically ordered summary of literature review**

<b>Database</b>	<b>Date</b>	<b>Total of Entries</b>	<b>Saved for further research</b>
EconLit	18/04/2013	365	59
Social Science Citation Index and Conference Proceedings Citation Index	24/04/2013	249	34
IngentaConnect	25/04/2013	982	12
Environmental Valuation Reference Inventory	30/04/2013	228	11
AGRICOLA. US National Agricultural Library Catalog	02/05/2013	143	0
SSRN	02/05/2013	285	16
ProQuest	03/05/2013	3,776	32
<b>Total</b>		<b>6,028</b>	<b>164</b>

Source: Own elaboration.

In excess of 6,000 records from various electronic databases were scrutinised and 164 studies earmarked for further inspection. A further update was undertaken in May 2014 to guarantee that any recently published studies were also included (resulting in an additional four studies). The rules for inclusion of studies into the meta-analysis are identical to those employed by Daniel et al. (2009a), chiefly:

- (i) Estimates have necessarily to be obtained using an econometrically estimated HPF, either the DID or the standard hedonic model.

- (ii) Estimates must be capable of being expressed, after recalculation if necessary, as a percentage of average house prices.
- (iii) The risk of flooding should be captured by a dummy variable, where the dummy variable indicates location within the 500-year or 100-year floodplain.
- (iv) For those studies which are available in more than one version of a paper only the most recent version is considered.

Adhering to these rules we compile a meta-sample which includes only those studies which are sufficiently homogenous for meaningful conclusions to be obtained through meta-analysis. These rules however, imply that certain sorts of studies were excluded. The first rule excludes those studies with estimates that were obtained through a comparison of average sale prices both outside and within a floodplain, such as Babcock and Mitchell (1980) and Zimmerman (1979), studies comparing rates of property price appreciation, such as Lamond et al. (2010) or Eves (2002) and those studies utilising repeat-sales models, such as Carbone et al. (2006) and Lamond and Proverbs (2006).<sup>3</sup> The second rule necessitates dropping studies that provide monetary estimates of the change in the price associated with floodplain location but fail to provide information on the mean house price e.g. Holway and Burby (1990). The third rule excludes studies such as Barnard (1978), Tobin and Montz (1994) and Shilling et al. (1985) who use the cost of flood insurance, elevation or flood depth rather than floodplain location as an indicator for flood risk. Also excluded is the study of Atreya and Ferreira (2012b) who use a solitary dummy variable in order to identify those properties that are located in a floodplain irrespective of designation, in addition to some results from studies by Bin and Landry (2013) and Bin et al. (2008b) where the extent of the risk is unspecified.

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<sup>3</sup> Repeat sales models value only the change in the price of property located in the floodplain following an 'informational update' i.e. a flood event. In other words, using results from repeat-sales models we are only be able to recover an estimate of  $\theta$  as expressed in equation (5) which corresponds to the price update after a flood. Without information on  $\gamma$  (the pre-flood price differential) we are unable to compute the post-flood price differential.

The resulting meta-sample contains 37 studies and 349 point estimates, twice the studies considered by Daniel et al. (2009a) and three times the number of point estimates. The publication dates of studies contained in the meta-sample ranges from 1987 to 2013. The meta-sample contains 33 studies from the United States as well as studies from Australia, New Zealand, the Netherlands and the United Kingdom (as shown in Table 2).

Whilst all the studies use dummy variables controlling for floodplain location the functional form selected for the HPF differs from study to study, and various adjustments were required. The effect size of interest is the relative house price differential for floodplain location and, adopting the notation of Daniel et al. (2009a), we refer to this as  $T$ , with  $s_T$  the associated standard errors. The majority of studies employ a semi-log functional form as in equation (7), the variables are defined as in equation (5).<sup>4</sup> Here, the effect size  $T$  and the standard error  $s_T$  correspond to the parameter  $\gamma$  and the standard error  $s_\gamma$  as extracted direct from the primary studies themselves.<sup>5</sup> Studies by Donnelly (1989), Speyrer and Ragas (1991), Bialaszewski and Newsome (1990), US Army Corps of Engineers (1998), Shultz and Fridgen (2001) and Harrison et al. (2001) by contrast provide estimates that are taken from linear specifications such as those in equation (8) so that  $T = \gamma/\bar{P}$  and  $s_T = s_\gamma/\bar{P}$ , where  $\bar{P}$  is the mean selling price of the sample.

$$\ln P_i = \alpha_0 + \gamma FPD_i + \sum_{j=1} \alpha_j Z_{ij} + \varepsilon_i \quad (7)$$

$$P_i = \alpha_0 + \gamma FPD_i + \sum_{j=1} \alpha_j Z_{ij} + \varepsilon_i \quad (8)$$

$$\frac{P_i^\lambda - 1}{\lambda} = \alpha_0 + \gamma FPD_i + \sum_{j=1} \alpha_j Z_{ij} + \varepsilon_i \quad (9)$$

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<sup>4</sup> For convenience we exclude the variable  $r_i$ .

<sup>5</sup> Note that, strictly, with a dummy variable contained in a semi-log functional form the marginal effect ought to be adjusted to  $e^\gamma - 1$  (Halvorsen and Palmquist, 1980). Despite this, following Daniel et al. (2009a) these adjustments are ignored given the very small size of the coefficients.

Studies by MacDonald et al. (1987), Dei-Tutu and Bin (2002), MacDonald et al. (1990) and Bin (2004) resort to a Box-Cox specification such as that shown in equation (9). Here  $T = \gamma \bar{P}^{1-\hat{\lambda}}$ , where  $\bar{P}$  represents the average price and  $\hat{\lambda}$  is a non-linear parameter. In this instance  $T$  depends upon two random parameters and accordingly  $s_T$  cannot readily be calculated from the reported parameters. Using the same strategy as Daniel et al. (2009a) the standard errors have been approximated by means of the Delta method, as shown in equation (10).

$$s_T = \sqrt{\left(\frac{\partial T}{\partial \lambda}\right)^2 \sigma_\lambda^2 + \left(\frac{\partial T}{\partial \gamma}\right)^2 \sigma_\gamma^2 + 2 \left(\frac{\partial T}{\partial \lambda}\right) \left(\frac{\partial T}{\partial \gamma}\right) r_{\gamma\lambda} \sigma_\gamma \sigma_\lambda} \quad (10)$$

Here  $\sigma_i$  denotes the standard error of  $\lambda$  and  $\gamma$ , respectively, and  $r_{\gamma\lambda}$  is the associated correlation coefficient. For studies like MacDonald et al. (1990) and MacDonald et al. (1987) which fail to provide any estimate of  $\sigma_\lambda$ , this is approximated with a standard error of  $\lambda/2$  as in Daniel et al. (2009a); a step which makes  $\lambda$  significant at the 5 percent level of confidence. Since an estimate of  $r_{\gamma\lambda}$  is typically unavailable, the value of  $\pm 0.9$  has been inserted depending on whether the expression  $(\partial T/\partial \gamma)(\partial T/\partial \lambda)$  is positively or negatively signed to generate conservative standard errors.

All the estimates used in our analysis are based on actual sales data; 13 estimates from a study conducted by the US Army Corps of Engineers (1998) based on appraised values are excluded whilst other estimates from the same study are retained.<sup>6</sup> For studies using a spatial lag we record the total effect of flood risk location meaning that any spatial spillovers arising out of the

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<sup>6</sup> These were, included in Daniel et al. (2009a).



prices of neighbouring properties are included.<sup>7</sup> Studies incorporating a spatial lag include Daniel et al. (2007), Posey and Rogers (2010), Bin et al. (2008a, 2008b), Atreya and Ferreira (2012a), Atreya et al. (2012), Atreya and Ferreira (2012c), Atreya et al. (2013) and Meldrum (2013).<sup>8</sup>

Finally, we collect information on the flood history of each study's location. Using this information we calculate time elapsed since the last flood. Some hedonic studies such as Bin and Landry (2013), Turnbull et al. (2013), Rambaldi et al. (2011), Bin et al. (2008a), Lamond and Proverbs (2006), Bin (2004), Bartosova et al. (1999) and US Army Corps of Engineers (1998) already discuss the flood history of the location. In these cases, months elapsed since the most recent flood is then easily calculated by subtracting the date of the most recent flood from the median date of the transaction data used in the study. For DID studies the time elapsed since the most recent flood is, for 'pre-flood' estimates, clearly different to that for 'post-flood' estimates. Since DID studies invariably mention the date of the flood event around which the study is constructed it is easy to calculate time elapsed for the post-flood estimates. For recent studies by Atreya et al. (2012, 2013), Atreya and Ferreira (2011, 2012a, 2012c) and Bin and Landry (2013) which investigate how the information content of flood events diminishes with the passage of time as in equation (6), the resulting post-flood price differential is the price discount for floodplain location immediately after the flood.

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<sup>7</sup> For studies using spatial econometric methods the total effect was calculated following Golgher and Voss (2016). In an earlier version of the manuscript for spatial studies we included only the direct effect. The results are largely unaffected by using the total effect rather than the direct effect.

<sup>8</sup> Note that the average effect sizes contained in Table 2 correspond to the relative house price differential for floodplain location and are not adjusted for differing levels of risk. In contrast Daniel et al. (2009a) standardised both the effect sizes as well as the corresponding standard errors in order to account for any differences in the degree of risk reporting them as  $T^* = T \times (1/\omega \times 100)^{-1}$ , where  $T$  denotes the unstandardised effect size and the recurrence interval is  $\omega$ . In our research no standardisation has been attempted and any differences arising out of differences in the levels of risk in our study are investigated as part of our meta-analysis.

To determine time elapsed since the previous flood for studies that do not discuss the flood history of the location as well as for the ‘pre-flood’ period of DID studies it was necessary to examine historical records. Using the google search engine we retrieved information from previous floods at the city or county level according to the area of interest in the primary studies. More specifically, we focus on identifying major flood events that could cause a significant impact on the price of properties. The search was conducted on a study by study basis based on four criteria: (1) the location of interest of the primary study, (2) the use of words such as “flood” or “inundation” to define the nature of the event, (3) the use of words such as “major”, “large” or “extreme”, to denote the magnitude of the flood, and (4) the date of occurrence of the flood before the start date of the sample. In general, the date of the previous flood is taken from official online reports of the local authority or reports from local news. The occurrence of the event was then confirmed using different sources to ensure a consistent description of the event as a “major” flood event. Table A1 in the appendix shows the date of the previous flood for each study considered in the meta-analysis. The number of months elapsed since the previous flood was calculated by subtracting the date of the previous flood from the median date of the transaction data used in the study.<sup>9</sup>

It is interesting to note that, although all primary studies investigate properties located in either the 100-year or the 500-year floodplain, according to the evidence we have compiled the time elapsed since the last flood event is surprisingly short. In those hedonic studies which do not observe a flood event during the period of the study, the average amount of time elapsed since

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<sup>9</sup> Note that there are 5 studies where the “event” corresponded to a change in law or regulation (Samarasinghe and Sharp, 2010; Troy and Romm, 2004; Pope, 2008; Troy, 2001; Harrison et al., 2001). When collecting the data from these studies we handled them in the same way as we handled the estimates of a flood. That is for the post-disclosure effect we add a pre-disclosure discount plus the update after disclosure. In this way, the disclosure is considered similar to the occurrence of a flood in that both events provide new information with which individuals are able to update their perception of risk. In this case, however, the date of the previous flood is the same for the pre- and post-disclosure effects.

the most recent flood was 15.8 years. For the post-flood component of the DID study the average time elapsed was 2.5 years. But whereas choosing a location with a recent flood event is necessary for the conduct of a DID study even for these studies there seems to have been a prior flood event occurring shortly before the ‘main’ event. On average a prior flood occurred 21.2 years before the midpoint of the pre-flood period of DID studies.

Either researchers have chosen locations subject to recent flooding or these locations contain low-lying areas which are in fact subject to flooding far more frequently than their 100-year or 500-year floodplain designation suggests. We suspect part of the explanation is that a 100-year floodplain is defined by the risk at the edge of the floodplain. Highfield et al. (2013) and Czajkowski et al. (2013) note floodplain designations are neither an accurate nor sufficient indication of the level of risk because houses are treated equally regardless of their distance to the source of risk. Within a 100-year floodplain there are properties which can be affected by floods with a shorter return period e.g. of 1 in 50 years, and individuals with a different perception of risk (O’Neil et al. 2016; Ho et al. 2008). Despite this we suspect researchers are drawn to conduct analyses in locations with a recent flood history. In situation in which the price discount for locating within a 100-year floodplain is partially determined by the time elapsed since the most recent flood event a different set of results might be obtained from randomly selecting floodplains rather than concentrating on those recently flooded.

**Table 2. Summary of studies included in the meta-sample**

No.	Authors	Year	Country <sup>1</sup>	Location	Flood risk (floodplain)	No. Obs.	Effect size ( <i>T</i> )			
							Mean	S.D.	Min.	Max.
1	MacDonald et al.	1987	US	Louisiana	100	2	-0.077	0.014	-0.086	-0.067
2	Skantz and Strickland	1987	US	Texas	100	8	-0.025	0.019	-0.056	-0.012
3	Donnelly	1989	US	Wisconsin	100	1	-0.121	-	-	-
4	Shilling et al.	1989	US	Louisiana	100	1	-0.076	-	-	-
5	Bialszewski and Newsome	1990	US	Alabama	100	1	0.000	-	-	-
6	MacDonald et al.	1990	US	Louisiana	100	2	-0.100	0.024	-0.117	-0.083
7	Speyrer and Ragas	1991	US	Louisiana	100	4	-0.098	0.073	-0.204	-0.042
8	US Army Corps of Engineers	1998	US	Texas	100	14	-0.029	0.083	-0.268	0.080
9	Bartosova et al.	1999	US	Wisconsin	100 and 500	7	-0.016	0.074	-0.078	0.144
10	Harrison et al.	2001	US	Florida	100	4	-0.025	0.013	-0.041	-0.014
11	Shultz and Fridgen	2001	US	ND and MI <sup>2</sup>	100 and 500	4	-0.032	0.073	-0.102	0.031
12	Troy	2001	US	California	100	20	0.024	0.022	-0.017	0.061
13	Dei-Tutu and Bin	2002	US	North Carolina	100	1	-0.062	-	-	-
14	Bin	2004	US	North Carolina	100	4	-0.062	0.015	-0.076	-0.044
15	Bin and Polasky	2004	US	North Carolina	100	3	-0.060	0.023	-0.084	-0.038
16	Troy and Romm	2004	US	California	100	2	-0.011	0.030	-0.032	0.009
17	Hallstrom and Smith	2005	US	Florida	100	8	0.066	0.118	-0.113	0.173
18	Bin and Kruse	2006	US	North Carolina	100 and 500	9	0.107	0.235	-0.103	0.610
19	Lamond and Proverbs	2006	UK	North Yorkshire	100	2	-0.175	0.005	-0.178	-0.171
20	Daniel et al.	2007	NL	Meuse River	100	15	-0.033	0.054	-0.082	0.084
21	Morgan	2007	US	Florida	100	3	0.254	0.080	0.165	0.321
22	Bin et al.	2008a	US	North Carolina	100	2	-0.146	0.026	-0.165	-0.128
23	Bin et al.	2008b	US	North Carolina	100 and 500	6	-0.055	0.031	-0.078	-0.010
24	Pope	2008	US	North Carolina	100 and 500	22	-0.002	0.025	-0.045	0.038
25	Daniel et al.	2009b	NL	Meuse River	100	4	-0.049	0.041	-0.086	0.005
26	Kousky	2010	US	Missouri	100 and 500	46	-0.024	0.017	-0.073	0.008
27	Samarasinghe and Sharp	2010	NZ	Auckland	100	4	-0.040	0.025	-0.064	-0.014
28	Posey and Rogers	2010	US	Missouri	100	2	-0.099	0.002	-0.100	-0.098
29	Atreya and Ferreira	2011	US	Georgia	100 and 500	6	-0.134	0.143	-0.375	0.042
30	Atreya and Ferreira	2012c	US	Georgia	100	20	-0.187	0.245	-0.722	0.127
31	Atreya and Ferreira	2012a	US	Georgia	100 and 500	18	-0.174	0.195	-0.677	0.102
32	Atreya et al.	2012	US	Georgia	100 and 500	22	-0.084	0.164	-0.382	0.101
33	Rambaldi et al.	2013	AU	Queensland	100	1	-0.013	-	-	-
34	Atreya et al.	2013	US	Georgia	100 and 500	40	-0.166	0.229	-0.755	0.089
35	Bin and Landry	2013	US	North Carolina	100 and 500	18	-0.096	0.113	-0.423	0.041
36	Meldrum	2013	US	Colorado	100	21	-0.057	0.060	-0.158	0.010
37	Turnbull et al.	2013	US	Louisiana	100 and 500	10	-0.006	0.016	-0.023	0.014
<b>Overall</b>						<b>349</b>	<b>-0.061</b>	<b>0.149</b>	<b>-0.755</b>	<b>0.610</b>

Notes: <sup>1</sup> AU = Australia, NL = The Netherlands, NZ = New Zealand, UK = United Kingdom, US = United States.

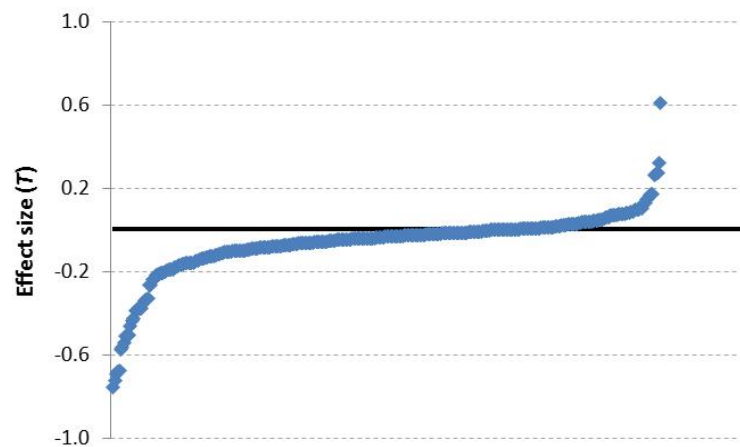
<sup>2</sup> ND = North Dakota, MI = Minnesota.

#### 4. Meta-Analysis

According to Table 2 there is general agreement that the price of property located in the floodplain is lower than that of an equivalent property outside; 33 out of 37 studies report, on average, a depressing effect of floodplain location. Properties situated inside a floodplain are

reduced in value by on average  $-6.1$  percent although there is significant within and between study variation as revealed by the columns recording the standard deviations (SD) of the effect sizes and the minimum and maximum effect size. Estimates range from the  $-75.5$  percent price discount found by Atreya et al. (2013) to the  $+61.0$  percent price premium reported by Bin and Kruse (2006). Figure 1 displays the 349 estimates of the effect size included in our meta-sample.

**Figure 1. Meta-sample: Relative price differential for location in the floodplain**



Source: Authors' own elaboration founded on results from primary studies.

We now perform a meta-analysis of the estimated effect sizes. There are in the literature two commonly encountered models used for combining together effect-sizes: the fixed-effects and random-effects models. In either weights are usually chosen using the (inverse) error-variance with the consequence that greater weight is given to more precise studies. The essential difference between these two models resides in the assumptions that define the error variance.

With the fixed-effects model the assumption is that all studies possess a common effect size; any differences in the observed effects occur only because of sampling error. Alternatively put, if the sample size was infinite the observed effect would be the same for all studies. However, because studies commonly differ in terms of both the way that they are implemented as well as the underlying population the assumption of the fixed-effects is here implausible. By contrast the

random-effects model allows the real effect size to differ across observations. In this case the goal is to estimate the mean of the distribution of true effect sizes.

Table 3 reports the statistics from the random-effects model estimated on all 349 effect sizes. Also reported are results from different subsamples where observations have been put into categories according to the floodplain designation, inland versus coastal flooding and whether it is a before or after-the-flood DID estimate or an estimate from a standard hedonic model. The table additionally reports the between-study variance  $\tau^2$ , the 90 percent confidence intervals, the  $I^2$  and the  $Q$ -statistic.

So far we have treated each observation as if it were a separate study, something which results in inappropriate weight being placed on those studies reporting in excess of one outcome. To rectify this shortcoming, Table 3 also shows the summary statistics invoking another common weighting scheme: conferring weights according to a study's sample size. The weight given to each particular observation corresponds to the square root of the mean sample size of each study, divided by however many estimates each study contributes to the final meta-sample. Hence studies are given more weight because of the information they contain and not simply because the researchers happened to report more estimates (Stanley and Doucouliagos, 2015).

The overall effect size for estimates in the 100-year floodplain points to a premium of +3.7 percent rather than the expected price discount. This premium however, is attributable to properties threatened by coastal flooding. If we further divide the sample the overall effect size for properties affected by inland flooding points to a discount of -5.6 percent but for properties at risk of coastal flooding a premium of +14.8 percent. Bin and Kruse (2006), Hallstrom and Smith (2005) and Bin et al. (2008a) suggest such results are due to the failure to control for the presence of amenities associated with proximity to the coast. Using an equilibrium sorting model, Fan and Davlasheridze (2016) also find evidence that amenity values dominate flood risk

in coastal regions. We believe that it is at present impossible to draw reliable inferences from studies carried out in coastal regions.

For properties subject to inland flooding in the 100-year floodplain, if we focus only on the evidence from DID models before the flood event, there is a sizeable discount of −2.9 percent. After a flood the discount rises to −6.9 percent. For houses in the 500-year floodplain, before a flood event DID models suggest that the price differential is +0.3 but after a flood event there is a discount of −5.2 percent.<sup>10</sup> This finding supports the idea that there is a significant updating of beliefs in localities where no prior capitalisation of flood risk has occurred (Kousky, 2010).

**Table 3. Meta-analysis: Summary statistics**

Sample	Random-Effects						Sample size weights			
	N	Summary Statistic <sup>1</sup>	90% Conf. Interval	$\tau^2$	Q-Stat <sup>2</sup>	I <sup>2</sup>	Summary Statistic <sup>1</sup>	90% Conf. Interval	Q-Stat <sup>2</sup>	I <sup>2</sup>
All	349	-0.027***	[-0.035; -0.020]	0.0032	5962.7***	94.2	-0.032***	[-0.037; -0.027]	6940.3***	95.0
500 year	93	-0.001	[-0.006; 0.005]	0.0001	133.9***	31.3	-0.019***	[-0.030; -0.009]	374.7***	75.4
100 year	256	-0.036***	[-0.046; -0.027]	0.0042	5736.4***	95.6	0.037***	[ 0.043; 0.032]	6569.1***	96.1
Inland	314	-0.028***	[-0.033; -0.022]	0.0011	2132.7***	85.3	-0.044***	[-0.049; -0.038]	3528.6***	91.1
Inland 100-year	226	-0.038***	[-0.044; -0.031]	0.0013	1794.7***	87.5	-0.056***	[-0.062; -0.050]	3248.6***	93.1
DID Inland 100-year BF	63	-0.017***	[-0.029; -0.006]	0.0011	239.4***	74.1	-0.029***	[-0.039; -0.020]	12023.1***	99.9
DID Inland 100-year AF	71	-0.079***	[-0.101; -0.058]	0.0039	222.4***	68.5	-0.069***	[-0.088; -0.051]	257.2***	72.8
Inland 500-year	88	0.003	[-0.002; 0.007]	0.0000	104.5*	16.7	-0.019***	[-0.030; -0.008]	347.1***	74.9
DID Inland 500-year BF	31	0.005	[-0.003; 0.013]	0.0000	14.7	0.0	0.003	[-0.008; 0.014]	1512.5***	98.0
DID Inland 500-year AF	32	-0.025***	[-0.041; -0.009]	0.0000	14.6	0.0	-0.052***	[-0.078; -0.027]	26.24	0.0
Coast	35	0.028	[-0.032; 0.089]	0.0308	2074.2***	98.4	0.134***	[ 0.122; 0.146]	2076.9***	98.4
Coast 100 year	30	0.045	[-0.019; 0.109]	0.0298	1813.7***	98.4	0.148***	[ 0.134; 0.161]	1819.4***	98.8
DID Coast 100-year BF	7	0.118	[-0.030; 0.265]	0.0391	841.3***	99.3	0.241***	[ 0.229; 0.253]	969.7***	99.4
DID Coast 100-year AF	7	0.016	[-0.072; 0.105]	0.0110	43.02***	86.1	0.100***	[ 0.067; 0.132]	45.47***	86.8
Coast 500-year	5	-0.066***	[-0.092; -0.040]	0.0000	1.35	0.0	-0.069***	[-0.100; -0.037]	1.40	0.0
Hedonic	138	-0.030***	[-0.040; -0.021]	0.0023	3383.3***	96.0	-0.039***	[-0.043; -0.034]	4299.4***	96.8
DID Hedonic	211	-0.033***	[-0.047; -0.019]	0.0067	2383.9***	91.2	-0.027***	[-0.035; -0.019]	2931.9***	92.8
DID Hedonic BF	101	-0.004	[-0.023; 0.015]	0.0070	1928.7***	94.8	-0.001	[-0.008; 0.006]	2071.7***	95.2
DID Hedonic AF	110	-0.060***	[-0.078; -0.043]	0.0039	339.0***	67.8	-0.053***	[-0.067; -0.039]	381.8***	71.5

Notes: <sup>1</sup> H0: that the summary effect size is insignificantly different from zero. <sup>2</sup> H0: that all studies contained in the sample share a single effect size.

\*, \*\* and \*\*\* implies rejection of the null at the 10 percent, 5 percent and 1 percent significance level.

BF= Before flooding event, AF= After flooding event.

<sup>10</sup> It has been pointed out to us that insurance is mandatory for houses in high risk areas in the US when the house is purchased using a mortgage from a federally regulated lender.

## 5. Publication Bias

So far, our meta-analysis has assumed the effect sizes from published studies constitutes a representative sample of the population of all possible studies, notwithstanding the possible tendency of researchers to conduct studies in locations with a recent history of flooding. The chance of sample selection has hitherto been ignored. Since De Long and Lang (1992), publication bias has however been acknowledged as an important issue in empirical work. Card and Krueger (1995) list three possible sources of selection bias:

1. Editors and reviewers might be inclined to accept papers with findings consistent with economic theory.
2. Researchers might use as a criterion for model selection the presence of an expected result.
3. A general tendency to view statistically significant results in a more favourable light.

We have avoided adding to this problem by undertaking a comprehensive review and by incorporating evidence irrespective of significance and the sign of the effect. However, as noted publication bias could originate from other sources, and hence our efforts might eliminate some, but not all the potential bias. The purpose of this section is to gauge whether publication bias is present in the literature.

Publication bias results from selective sampling. The literature identifies two main kinds of publication bias:

- Type I.** Directional: Selection supports a particular effect, e.g. a negative or positive effect.
- Type II.** Statistical significance: Selection favours statistically significant results, irrespective of their sign.



Meta-regression is now a popular means of identifying publication bias, in particular by testing for funnel plot asymmetry. Card and Krueger (1995) illustrate this technique by examining the effect of minimum wage legislation on employment. At its simplest, meta-regression for testing and correcting for publication bias takes the form of a regression between the effect sizes and their associated standard errors, as in equation (11).

$$T_i = \beta_0 + \beta_1 s_{Ti} + \varepsilon_i \quad (11)$$

In the absence of selection, effect sizes ought to vary randomly around  $\beta_0$ , and be independent of the standard error. Yet as Egger et al. (1997) notes when publication bias is present it is revealed by the sign and statistical significance of  $\beta_1$ , whereas  $\beta_0$  can be regarded as the true effect size, after controlling for publication bias. The meta-regression is however frequently transformed because the error term  $\varepsilon_i$  will be heterosedastic because primary studies use different sample sizes. Accordingly, a variant of equation (11) is typically used to obtain superior estimates. Dividing by the estimated standard errors results in equation (12). Stanley (2005) shows this procedure eliminates both sorts of publication bias noted above.

$$T_i/s_{Ti} = \beta_1 + \beta_0(1/s_{Ti}) + e_i \quad (12)$$

Table 4 reports the results for equation (12). Column 1 displays the results using the entire sample of effect sizes. We report Huber-White standard errors to account for remaining heteroskedasticity. A  $t$ -test on the intercept,  $\beta_1$ , points to significant publication bias with a tendency to reporting more negative impacts. Egger et al. (1997) suggest the power of this test is restricted and prefer to base evidence of asymmetry on the one tailed  $t$ -test with  $p < 0.1$ . Results of this second test are provided in square brackets in Table 4. This test also reveals evidence of

publication bias. Additionally, the coefficient  $\beta_0$  seems to suggest that the effect of flood risk on house prices is insignificant. However, the coefficients contained in column 1 weight all of the effect sizes as if wholly independent studies; something which results in an overrepresentation of particular studies contributing more than one observation. To address this issue we again assign weights in proportion to the square root of each study's sample size divided by however many estimates it contributes. Column 2 displays the results with weighting for the entire sample, and columns 3-7 report coefficients for different samples.

**Table 4. Meta-regression: The Funnel Asymmetry Test**

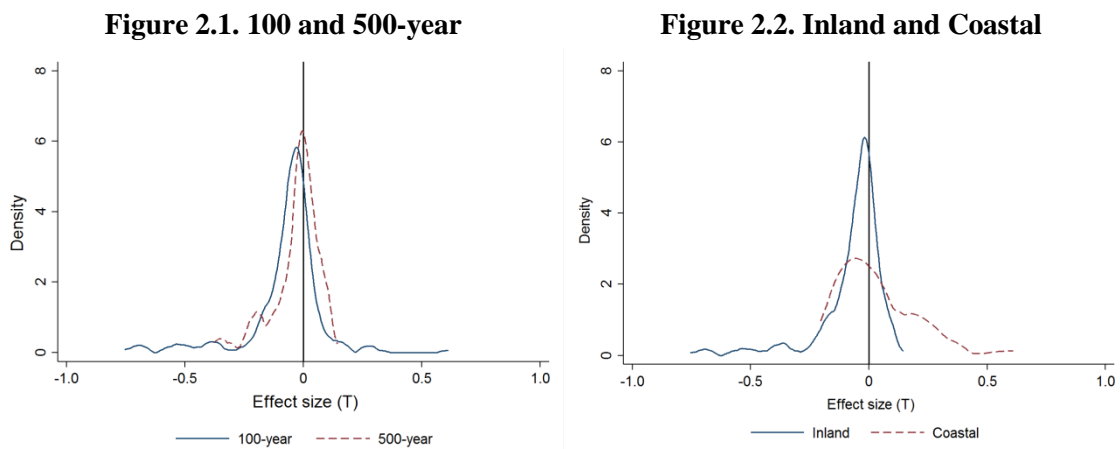
Variables	Sample size weights						
	(1) Full Sample	(2) Full Sample	(3) 100 year	(4) 500 year	(5) Inland	(6) Inland 100 year	(7) Coast
$1/Se_i (\beta_0)$	-0.002434 (0.00727)	-0.0404 (0.0253)	-0.0507* (0.0271)	0.00815*** (0.00188)	-0.0517** (0.0206)	-0.0633*** (0.0202)	0.363*** (0.0255)
Constant ( $\beta_1$ )	-0.707*** (0.212)	1.093 (1.008)	1.479 (1.102)	-0.956*** (0.128)	0.831 (0.892)	0.793 (0.938)	-10.15*** (1.170)
$t \alpha = 0.10$	[0.000]***	[0.139]	[0.112]	[0.000]***	[0.176]	[0.199]	[0.000]***
Observations	349	349	256	93	314	226	35
R-squared	0.001	0.097	0.121	0.222	0.356	0.472	0.929
Rmse	4.109	8.284	9.705	0.835	4.747	4.870	5.002

Note: The dependent variable corresponds to the standardised effect size, i.e. the corresponding t-value. Standard errors in parentheses are Huber-White robust. Numbers in square brackets correspond to p-values for the one-tailed t-test. \*, \*\* and \*\*\* means rejection of the null at the 10 percent, 5 percent and 1percent level of significance.

After accounting for the overrepresentation of studies contributing multiple estimates, the results in column 2 reveal that there is now no longer any evidence of publication bias in the literature as a whole. Critically however, the coefficient  $\beta_0$  is at the same time insignificantly different from zero; something suggesting that the effect of flood risk on house prices is imperceptible when publication bias is removed.

Once we consider different subsamples results change. There is no evidence of publication bias for studies looking at the 100-year floodplain, inland flooding or inland flooding in the 100-year floodplain. By contrast, there is evidence of publication bias for studies examining the 500-year floodplain and coastal flooding. After correcting for publication bias properties located in coastal floodplains enjoy a price premium of 36.3 percent, although, as discussed, it is almost certain that this results from a failure to adequately control for any amenities associated with nearness to the coast. Figure 2 displays, by the level and type of risk, the distribution of effect sizes. As expected, the distribution from properties located in the 100-year floodplain displays a larger discount, when compared to those properties located in the 500-year floodplain. Figure 2.2 shows that most, if not all, of the high premiums for the 100-year floodplain estimates in figure 2.1 correspond to results from coastal regions.

**Figure 2. Effect size: Distribution density plots for different types of risk and different levels of risk.**



Source: Authors' own elaboration based on the results of the primary studies.

## 6. Meta-Regression Analysis

Our database comprises 50 variables describing the specific attributes of every study. However, it is impossible to incorporate all of them in the meta-regression since the necessary information is not always available. As a consequence, a set of 18 explanatory variables was included in the regression. These are described in Table 5. The first and for us most interesting variable controls for changes in flood risk perception caused by the amount of time elapsed since the last flood. The second group of variables controls for whether the study relates to a 100-year or 500-year floodplain. Following Daniel et al. (2009a) this variable is coded 0.002 for properties that are located in a 500-year floodplain and 0.01 for properties located in a 100-year floodplain. Making the assumption that any change in the objective risk of flooding *ceteris paribus* results in an equal change in the subjective risk of flooding we thus identify the relationship between the house price discount and the subjective risk of flooding from the inter-study variation in the objective risk of flooding. More specifically the coefficient on this variable can be interpreted as the percentage discount for houses located in the 100-year floodplain.

The third set of moderator variables accounts for context. It includes the log of the mean square footage of houses included in each primary study (*lav\_feet*) in order to control for dissimilarities in the kind of houses and the log of the mean price of a house in 2010 USD (*lavprice\_2010*) serving as a proxy for the incomes of households and for any income differences across studies. As Carbone et al. (2006) and Hallstrom and Smith (2005) point out, following a flood the price discount is expected to be more substantial because some homeowners are likely to have suffered flood damages; hence we include a dummy variable identifying studies which explicitly state that their estimated coefficient corresponds to properties that have been flooded (*flooded*). Similarly, following Pryce et al. (2011), we anticipate a somewhat higher discount for houses in the floodplain in the aftermath of a second successive flood (*scnd\_flood*). Also included is a dummy variable (*dd\_afterlaw*) that is used to identify the effect sizes drawn from those studies looking at the price differential for floodplain location following any changes in regulations and last of all a dummy variable (*coast*) is used to identify the effect sizes of studies undertaken in coastal locations.

A fourth set of moderator variables comprises two dummy variables representing the presence of a range of controls in primary studies. The first of these (*amenity*) assumes the value unity for studies that include variables controlling for proximity to water. As already mentioned, if amenity values that correlate with flood risk are omitted we expect resulting estimates of the value of risk to be biased. A second dummy (*real\_p*) identifies those studies that explicitly use constant house prices in an attempt to control for time trends.<sup>11</sup> The fifth set of moderator variables refers to econometric differences exhibited by the primary studies. Two dummies (*linear* and *Box-Cox*) account for any differences in the functional form of the HPF; the omitted category corresponds to a semi-log functional form. A further dummy variable (*spatial*) highlights the use of spatial econometrics and the variable *dd\_hpm* assumes the value unity for any estimates from DID models, either before or after a flood event.

The final set of moderator variables controls for miscellaneous features of primary studies. In order to account for the quality of each study, we include a dummy (*published*) taking the value unity to distinguish studies published in refereed journals from working papers, dissertations or conference proceedings. The median sample year of each primary study (*med\_sampleyear*) is included to identify time trends in the effect size; we also include the time span (*time\_span*) of every study. Table 5 includes summary statistics for all these variables.

Recall that in our meta-regression the weight assigned to every observation is proportionate to the square root of the sample size divided by the number of estimates contributed by each study.<sup>12</sup> Lewis and Linzer (2005) show that, when used in conjunction with heteroskedastic consistent standard errors, this technique yields very satisfactory results. We use the Huber-White variance estimator (White, 1980) accounting for both heteroskedasticity and any correlation between effect sizes drawn from the same study (Williams, 2000).

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<sup>11</sup> In a small number of instances it was not possible to establish whether the study did indeed use constant house prices.

<sup>12</sup> The meta-regression presented by Daniel et al. (2009a) uses three different weighting schemes. First they use a random-effects model (mixed-effects) where the weights are as stated above. Secondly, they present an unweighted model using Huber-White standard errors robust to heteroskedasticity and cluster correlation among effect sizes drawn from the same primary study. Third, they present results using inverse-variance weights in a fixed-effect model with Huber-White standard errors. All of these models treat observations as if they were a separate study resulting in the abovementioned problem of assigning improper weight to studies contributing multiple estimates of the effect size.

**Table 5. Description of variables included in the meta-regression model**

Variable	Description	Summary statistics				
		No. Obs.	Mean	St. dev.	Min.	Max.
<i>Dependent variable</i>						
Effect size (T)	Relative price differential for floodplain location.	349	-0.061	0.149	-0.755	0.610
<i>Flood risk perception</i>						
months	Number of months elapsed since the previous flood.	349	144.4	175.7	3.0	840.0
<i>Flood risk</i>						
Flood Risk	Variable = 0.01 if the effect refers to the 100-year floodplain and 0.002 for a 500-year floodplain.	349	0.0079	0.0035	0.002	0.01
<i>Context of the study</i>						
lav_feet	Natural log of the mean square feet of the properties per study.	349	7.425	0.249	6.558	8.051
lavprice_2010	Natural log of the mean price of the houses per study in 2010 US dollars.	349	11.861	0.557	9.191	12.956
flooded	Dummy = 1, if the effect refers to flooded properties.	349	0.025	0.155	0	1
scnd_flood	Dummy = 1, if the effect refers to a second flood.	349	0.033	0.179	0	1
dd_after	Dummy = 1, if the effect corresponds to a post-flood DID estimate.	349	0.329	0.471	0	1
dd_afterlaw	Dummy = 1, if the effect is from a DID model following a change in a regulation for floodplain designated areas.	349	0.074	0.262	0	1
coast	Dummy = 1, if the study area has a coastline.	349	0.102	0.303	0	1
<i>Control variables of study</i>						
amenity	Dummy = 1, if the study includes variables controlling for the amenity value of proximity to waterbodies.	349	0.876	0.330	0	1
real_p	Dummy = 1, if the study converts prices to a constant measure prior to estimation.	349	0.667	0.472	0	1
<i>Characteristics of econometric model</i>						
linear	Dummy = 1, if the effect corresponds to a linear specification of a hedonic price function.	349	0.071	0.258	0	1
Box-Cox	Dummy = 1, if the study utilises a semi-logarithmic HPF.	349	0.019	0.137	0	1
spatial	Dummy = 1, if the effect corresponds to a spatial econometric model.	349	0.343	0.475	0	1
dd_hpm	Dummy = 1, if the effect corresponds to a DID specification.	349	0.593	0.492	0	1
<i>Characteristics of the study</i>						
published	Dummy = 1, if the primary study is published in a refereed journal.	349	0.580	0.494	0	1
med_sampleyear	The median sample year of the study.	349	1995.1	6.464	1978	2006
time_span	The time span of the data covered in the study.	349	7.173	6.416	1	40

Source: Own elaboration based on estimates from primary studies.

Having addressed the issue of weighting in our meta-regression, we now test the hypothesis that the flood history of the locations matters and whether its inclusion means that there is no longer any statistically significant effect from differences in the objective risk. Following Atreya and Ferreira (2015), Atreya et al. (2013) and Bin and Landry (2013), we investigate four different transformations for the variable *months*: a linear specification ( $f(months) = months$ ), a logarithmic transformation ( $f(months) = \ln(months)$ ), a ratio specification ( $f(months) = (months - 1)/months$ ) and a square root specification ( $f(months) = \sqrt{months}$ ). These three transformations impose different degrees of curvature in the recovery of house prices following a flood.

The results of our meta-regression are shown in Table 6 where models 1-4 are the results obtained from assigning weights as above with different transformations of the *months* variable. In each of the models the coefficient on the variable *months* and transformations thereof is highly significant. These results indicate that as expected the price discount for floodplain location is much greater immediately following a flood, after which it starts to decay. Based on the goodness of fit criterion our preferred specification is model 3 which uses the ratio transformation of the variable *months*. This is similar to the preferred transformation of the time variable used by Bin and Landry (2013). Our results seem to suggest that results from earlier studies might be influenced as much by changes in subjective risk caused by an actual flood as by objective measures of flood risk.

Turning to the other main variable of interest differences in the objectively assessed risk are significant and highly consistent across the different specifications. In the preferred model 3 it appears that there is almost a -4.6 percent discount for being located in the 100-year floodplain compared to locations outside it. A number of other variables appear to have a consistent impact

on the effect size including whether the primary study used a linear functional form as well as regional dummies. The coefficients on the regional dummies are always positive and highly significant. These coefficients are measured with respect to Georgia, US, which is the omitted region in our regressions and for which primary studies report the highest discounts in our meta-sample. Specifically, Atreya and Ferreira (2012a, 2012b) and Atreya et al. (2013) analyse the impact of tropical storm Alberto in 1994 using DID hedonic models. This storm is considered to be among the worst flood disasters in the history of the US (The Albany Herald, 2014).

As a further test of robustness we include only those studies which are for inland flooding given our concerns about studies dealing with coastal flooding. In this approach, we follow DeCicca and Kenkel (2015) who suggest excluding from the meta-sample studies where the internal validity of the estimates could be compromised. In this case all 35 observations belonging to studies undertaken in coastal regions are dropped. The results are displayed in Table 7. We continue to find strong evidence that the effect size of floodplain location is greatest immediately after a flood but there remains a statistically significant effect of differences in floodplain designation pointing to a discount of  $-4.6$  percent for location in a 100-year floodplain. The only difference is that now the effect size depends on average house size too. Even if they are biased and suggest a premium for floodplain location including studies dealing with coastal flooding does not make any difference.



**Table 6. Meta-regression results**

Variables	Sample size weights				Characteristics of the study				
	(1) <i>mnths</i>	(2) <i>ln(mnths)</i>	(3) $\frac{(mnths - 1)}{mnths}$	(4) <i>Sqrt(mnths)</i>	published	-0.00860 (0.0189)	-0.0182 (0.0170)	-0.0160 (0.0156)	-0.0130 (0.0184)
<i>Flood risk perception</i>					med_sampleyear	0.00342* (0.00206)	0.00221 (0.00204)	-0.000760 (0.00210)	0.00351* (0.00208)
Mnths	0.000285*** (5.54e-05)	0.0479*** (0.00850)	1.117*** (0.210)	0.00937*** (0.00177)	time_span	0.00512** (0.00212)	0.000735 (0.00203)	0.00130 (0.00184)	0.00269 (0.00211)
<i>Flood risk level</i>					<i>Regional fixed effects</i> <sup>1</sup>				
Risk level	-4.748*** (0.665)	-4.683*** (0.628)	-4.573*** (0.629)	-4.748*** (0.639)	louisiana	0.162*** (0.0605)	0.266*** (0.0669)	0.168*** (0.0568)	0.234*** (0.0661)
<i>Context of the study</i>					n_carolina	0.160*** (0.0397)	0.171*** (0.0388)	0.106*** (0.0337)	0.178*** (0.0407)
lav_feet	0.0324 (0.0386)	0.0571 (0.0366)	0.0460 (0.0356)	0.0456 (0.0378)	texas	0.354*** (0.0531)	0.343*** (0.0501)	0.266*** (0.0455)	0.367*** (0.0535)
lavprice_2010	0.00503 (0.0555)	-0.0180 (0.0536)	-0.00506 (0.0533)	-0.00988 (0.0547)	wisconsin	0.216*** (0.0607)	0.204*** (0.0606)	0.131** (0.0532)	0.226*** (0.0624)
Flooded	-0.0303 (0.0623)	-0.0103 (0.0644)	-0.0255 (0.0633)	-0.0153 (0.0643)	alabama	0.453*** (0.0602)	0.455*** (0.0563)	0.371*** (0.0489)	0.473*** (0.0610)
scnd_flood	0.0218 (0.0348)	-0.0260 (0.0361)	-0.0623* (0.0355)	0.00680 (0.0360)	florida	0.352*** (0.0574)	0.375*** (0.0551)	0.326*** (0.0512)	0.374*** (0.0577)
dd_after	-0.0627** (0.0293)	-0.0338 (0.0280)	-0.0137 (0.0291)	-0.0523* (0.0288)	california	0.252*** (0.0547)	0.304*** (0.0570)	0.223*** (0.0503)	0.292*** (0.0572)
dd_afterlaw	0.0584** (0.0293)	0.0430* (0.0256)	0.0192 (0.0239)	0.0543* (0.0286)	missouri	0.118*** (0.0434)	0.104*** (0.0382)	0.0380 (0.0348)	0.128*** (0.0429)
coast	0.0416 (0.0331)	0.0336 (0.0310)	0.0306 (0.0310)	0.0388 (0.0320)	colorado	-0.00351 (0.0594)	0.0279 (0.0570)	0.0367 (0.0572)	0.00974 (0.0580)
<i>Control variables of study</i>					minnesota	0.355*** (0.0637)	0.373*** (0.0629)	0.357*** (0.0641)	0.366*** (0.0627)
amenity	-0.0112 (0.0235)	-0.0108 (0.0216)	-0.00816 (0.0201)	-0.00913 (0.0227)	nl	0.196*** (0.0623)	0.251*** (0.0620)	0.211*** (0.0571)	0.229*** (0.0628)
real_p	0.0551* (0.0306)	0.0937*** (0.0311)	0.0869*** (0.0307)	0.0732** (0.0306)	uk	0.0937** (0.0460)	0.171*** (0.0542)	0.110** (0.0493)	0.141*** (0.0509)
<i>Characteristics of econometric model</i>					aus	0.00794 (0.121)	0.173 (0.122)	0.105 (0.115)	0.106 (0.123)
linear	-0.165*** (0.0508)	-0.172*** (0.0470)	-0.179*** (0.0465)	-0.167*** (0.0493)	nz	0.0834 (0.0746)	0.0807 (0.0692)	0.0402 (0.0697)	0.0927 (0.0720)
Box-Cox	-0.0351 (0.0289)	-0.0387 (0.0274)	-0.0311 (0.0268)	-0.0397 (0.0275)	Constant	-7.362* (3.812)	-5.023 (3.775)	0.00363 (3.857)	-7.515* (3.866)
spatial	-0.0232* (0.0125)	-0.0186 (0.0119)	-0.0157 (0.0116)	-0.0215* (0.0123)	Observations	349	349	349	349
dd_hpm	0.0173** (0.00812)	0.0108 (0.00693)	0.0105 (0.00666)	0.0141* (0.00770)	R <sup>2</sup>	0.674	0.692	0.697	0.682
(Continued...)					Adj. R <sup>2</sup>	0.640	0.660	0.665	0.649
					Rmse	0.0544	0.0529	0.0525	0.0538

Note: <sup>1</sup> The omitted region is Georgia, US.

The dependent variable is *T* the effect size. Standard errors are in parentheses; for results employing sample size weights they correspond to Huber-White robust standard errors. \*, \*\* and \*\*\* means rejection of the null at the 10 percent, 5 percent and 1 percent significance level.

**Table 7. Meta-regression results: inland flood risk**

Variables	Sample size weights				Characteristics of the study				
	(1)	(2)	(3)	(4)	published	-0.0145	-0.0240	-0.0223	-0.0195
	<i>mnths</i>	$\ln(mnths)$	$\frac{(mnths - 1)}{mnths}$	$Sqrt(mnths)$		(0.0174)	(0.0158)	(0.0148)	(0.0169)
<i>Flood risk perception</i>					med_sampleyear	0.00135	0.000316	-0.00265	0.00145
mnths	0.000315*** (5.34e-05)	0.0451*** (0.00843)	1.066*** (0.213)	0.00974*** (0.00170)	time_span	(0.00198)	(0.00204)	(0.00210)	(0.00203)
<i>Flood risk level</i>						0.00277	-0.000739	-0.000401	0.000391
Risk level	-4.884*** (0.584)	-4.747*** (0.584)	-4.604*** (0.590)	-4.847*** (0.575)		(0.00221)	(0.00206)	(0.00187)	(0.00214)
<i>Context of the study</i>					<i>Regional fixed effects</i> <sup>1</sup>				
lav_feet	0.132*** (0.0312)	0.150*** (0.0298)	0.143*** (0.0284)	0.146*** (0.0309)	louisiana	0.0662	0.153***	0.0687	0.136**
lavprice_2010	-0.0196 (0.0626)	-0.0367 (0.0616)	-0.0296 (0.0602)	-0.0347 (0.0626)	n_carolina	(0.0556)	(0.0580)	(0.0509)	(0.0584)
flooded	-0.0133 (0.0584)	-0.00361 (0.0605)	-0.0172 (0.0586)	-0.00112 (0.0606)	texas	0.116*** (0.0377)	0.117*** (0.0379)	0.0580* (0.0347)	0.130*** (0.0392)
scnd_flood	0.00866 (0.0287)	-0.0379 (0.0295)	-0.0733** (0.0300)	-0.00774 (0.0296)	wisconsin	0.206*** (0.0414)	0.186*** (0.0354)	0.115*** (0.0414)	0.213*** (0.0387)
dd_after	-0.0353 (0.0282)	-0.0135 (0.0286)	0.00814 (0.0293)	-0.0265 (0.0283)	alabama	0.281*** (0.0492)	0.253*** (0.0445)	0.189*** (0.0419)	0.287*** (0.0484)
dd_afterlaw	0.00741 (0.0271)	0.000145 (0.0249)	-0.0239 (0.0222)	0.00537 (0.0269)	florida	0.341*** (0.0608)	0.324*** (0.0522)	0.255*** (0.0572)	0.354*** (0.0576)
coast	-	-	-	-	california	0.246*** (0.0606)	0.251*** (0.0510)	0.215*** (0.0560)	0.263*** (0.0564)
<i>Control variables of study</i>					missouri	0.167*** (0.0533)	0.209*** (0.0558)	0.141*** (0.0504)	0.205*** (0.0562)
amenity	-0.00743 (0.0180)	-0.00698 (0.0175)	-0.00771 (0.0171)	-0.00522 (0.0174)	colorado	0.150*** (0.0469)	0.124*** (0.0413)	0.0640* (0.0378)	0.157*** (0.0460)
real_p	-0.0880*** (0.0227)	-0.0424 (0.0272)	-0.0460 (0.0293)	-0.0675*** (0.0244)	minnesota	0.0692 (0.0660)	0.0893 (0.0627)	0.106* (0.0604)	0.0823 (0.0646)
<i>Characteristics of econometric model</i>					nl	0.246*** (0.0629)	0.247*** (0.0585)	0.246*** (0.0671)	0.251*** (0.0587)
linear	-0.175*** (0.0617)	-0.164*** (0.0576)	-0.180*** (0.0656)	-0.172*** (0.0582)	uk	0.0808 (0.0588)	0.138** (0.0580)	0.109** (0.0526)	0.117* (0.0601)
Box-Cox	0.00892 (0.0254)	0.00262 (0.0283)	0.00962 (0.0277)	0.00339 (0.0266)	aus	0.0281 (0.0379)	0.0945** (0.0457)	0.0426 (0.0430)	0.0740* (0.0425)
spatial	-0.00801 (0.00971)	-0.00536 (0.00986)	-0.00272 (0.00991)	-0.00696 (0.00974)	nz	-	-	-	-
dd_hpm	0.00683 (0.00611)	0.00245 (0.00595)	0.00188 (0.00552)	0.00424 (0.00609)	Constant	-3.552 (3.584)	-1.559 (3.719)	3.531 (3.772)	-3.727 (3.689)
(Continued...)					Observations	314	314	314	314
					R <sup>2</sup>	0.602	0.607	0.614	0.607
					Adj. R <sup>2</sup>	0.560	0.565	0.573	0.565
					Rmse	0.0465	0.0462	0.0458	0.0462

Note: <sup>1</sup> The omitted region is Georgia, US.

The dependent variable is  $T$  the effect size. Standard errors in parentheses; for results using sample size weights they correspond to Huber-White robust standard errors. \*, \*\* and \*\*\* means rejection of the null hypothesis at the 10 percent, 5 percent and 1 percent significance level.

## 7. Sensitivity analysis

Our findings point to a statistically significant difference in effect size depending on whether properties are located in the 100-year or 500-year floodplain, as well as to the importance of the amount of time elapsed since the last flood event. These findings moreover, are unaffected by the inclusion of studies undertaken in coastal locations, although such studies are subject to publication bias and point to a price premium for floodplain location. In this section we try to understand why our results are almost an order of magnitude different to the ones obtained by Daniel et al. (2009a). We also offer some advice for cost-benefit analysis of flood-relief projects. We begin however with a further test of robustness.

Earlier, we argued that all estimates of the floodplain discount are ‘post-flood’ estimates and that the only substantive difference is how much time has since elapsed. As a further test of robustness we divide the sample effect sizes into four groups: those derived from standard hedonic studies undertaken during a period in which there was no flooding, those from DID hedonic models (both before and after), the ‘pre-flood’ ones from DID studies and the ‘post-flood’ ones from DID studies. We then investigate whether the observed relationships between the floodplain discount, location in the 100-year rather than the 500-year floodplain and the amount of time elapsed change. These results appear in table A2 of the appendix for our preferred specification using the ratio transformation of the *months* variable ( $f(months) = (months - 1)/months$ ).

Analysing separately these observations yields a very similar set of coefficient estimates for the effect of location in the 100-year rather than the 500-year floodplain, as well as the effect of time elapsed for standard hedonic studies and DID estimates. But for the regression analysis using the ‘pre-flood’ subsample as well as for the ‘post-flood’ DID subsample none

of the coefficients on time elapsed are statistically significant. This is not however surprising. For ‘pre-flood’ DID estimates the average time elapsed with respect to the preceding flood is 21 years (254 months) which we believe is too long to capture any ongoing recovery of prices. For ‘post-flood’ DID studies by contrast, the sample is typically not long enough to draw inferences about the subsequent recovery in property prices.

We now compare the results of our meta-regression with those from Daniel et al. (2009a). Before doing so however, recall that being within a 100-year floodplain does not mean the probability of flooding therein is 1 percent per year. The reason is that a floodplain is defined by the probability of flooding only at its boundary. It would be wrong to interpret our results as suggesting that the effect of a 1 percent increase in the probability of flooding is equal to a –4.6 percent reduction in the price of a property. The correct interpretation of our results is that location inside a 100-year floodplain is associated with a discount of –4.6 percent.

In Daniel et al. (2009a) the authors regress effect size against an objective measure of risk which, as here, takes the value of 0.01 for location in a 100-year floodplain and 0.002 for location in a 500-year floodplain. The coefficient on this variable is, in their preferred specification, –0.6. They then use this finding to associate a –0.6 percent reduction in house prices with a 0.01 increase in the probability of flooding. But as we have just noted, this finding is more correctly interpreted as indicating that location inside a 100-year floodplain is associated with a –0.6 percent discount. Properly interpreted the –0.6 figure nevertheless sounds too small and is certainly very different from our estimate of –4.6.

To determine why their estimates are almost an order of magnitude different to ours we compiled the same set of observations from the same research papers used in Daniel et al.

(2009a). We also therefore now include those observations from the US Army Corps of Engineers (1998) referring to 50, 20 and 10-year floodplain locations which are based on appraisals. The results appear in table A3 of the appendix. Despite the fact that our control variables are slightly different to those used by Daniel et al. (2009a) we nonetheless obtain a similar, statistically insignificant estimate of  $-0.4$  percent for location in a 100-year floodplain, even when we use our preferred weighting method and include time elapsed since the last flood. But critically, when 37 observations relating to coastal flooding are removed the coefficient associated with the price discount for location in a 100-year floodplain increases to a statistically significant estimate of  $-2.7$  percent.

Combining coastal and inland studies in the same analysis is clearly problematical even if a dummy variable is used to identify studies in coastal locations. We observed earlier that studies in areas prone to coastal flooding are subject to significant publication bias and produce estimates pointing to a substantial price premium – a result others have attributed to failure to control for amenities associated with coastal location. Although Daniel et al. (2009a) note the problem of omitted variable bias and test for publication bias critically they do not distinguish between studies undertaken in coastal and inland floodplains. The question remains however, as to why including coastal studies in our meta-regression has almost no effect. The reason is that whereas in Daniel et al. (2009a) coastal studies contribute 41 percent of the observations in our study they contribute only 10 percent of the observations – most if not all of the recent research has dealt with inland locations.

It now remains only to recommend which estimate of the floodplain discount should be used in cost-benefit analyses of flood relief projects. In fact there are several candidates. Our meta-analysis of estimates of the floodplain discount produced an estimate of  $-5.6$  percent. This

estimate, which is based on 226 observations, is for location in the 100-year floodplain and refers exclusively to inland flooding. Another candidate is the  $-6.9$  percent discount observed immediately after a flood. Our preferred estimate however is taken from our meta-regression, once more limited to inland flooding and implicitly referring to location inside the 100-year floodplain, points to a  $-4.6$  percent discount based on 314 observations.

The chief difference between these estimates is that the third excludes short term impacts from recent floods whereas the former two ignore this source of heterogeneity. The first estimate has the advantage that it explicitly accounts for study heterogeneity whereas the second estimate is immediately after a flood when the consequences of flooding are most salient.

We suggest that the more conservative figure of  $-4.6$  percent be used as a rule of thumb for benefits estimation whenever a flood relief project in effect changes the boundaries of the floodplain. For example, if a project in effect removes a property from the 100-year floodplain placing it instead in the 500-year floodplain then the benefit of that project should be  $4.6 \times (0.01 - 0.002) = 0.037$  of the average price of property. Benefits will be greater for properties exposed to a higher level of risk. For instance, properties within the 25-year floodplain that are to be protected by a flood defence designed to prevent all but a 100-year event would enjoy benefits of  $4.6 \times (0.04 - 0.01) = 0.138$  of the average price of property. Obviously this figure does not consider potential positive or negative impacts associated with the defences e.g. visual intrusion or loss of access.

## 8. Conclusions

Economic theory suggests that housing markets provide a suitable means of measuring the benefits of flood risk reduction. Empirical evidence however indicates that properties within a 500-year or 100-year floodplain attract anything between a  $-75.5$  percent discount to a  $+61.0$  percent premium. This paper contains the results of a meta-analysis looking at the reported price discount for location inside the floodplain. The goal of the analysis is to provide answers to three questions: what is the most defensible point estimate of the price discount for floodplain location, is there any evidence of publication bias and what factors explain the observed variation in effect size?

Our results suggest there are important differences between alternative estimates of the effect-size. Dealing first with inland flooding, discounts for houses within a 100-year floodplain are  $-2.9$  percent rising to  $-6.9$  percent immediately after a flood. And although there seems to be a premium of  $+0.3$  percent for location in 500-year floodplains after a flood properties are discounted by  $-5.2$  percent. Such findings appear to confirm the view that recent floods cause homeowners to alter their perceptions of flood risk.

Evidence suggesting that properties exposed to coastal flooding enjoy a premium of  $+13.4$  percent seems to be a consequence of the correlation between floodplain location and the unrecorded amenities associated with proximity to the coast. Not only do studies into coastal flooding produce estimates of the percentage price discount with the ‘wrong’ sign there is also evidence of publication bias. Remarkably the results of the publication bias test suggest the real effect of flood risk in coastal regions after filtering out publication bias is even greater ( $+36.3\%$ ) and that there is a bias towards publishing results with smaller estimates. This leads us to agree with other researchers that studies of coastal floodplains are generally unreliable.

That researchers adopting a DID methodology are able to present ‘pre-flood’ and ‘post-flood’ estimates of the floodplain discount one tends to forget that all estimates have a prior flood history. We find that including a variable measuring time elapsed has considerable explanatory power. There is a significant recovery in house prices following a flood. But even after controlling for short term impacts objectively determined differences in floodplain designation have an impact equivalent to  $-4.6$  percent for location in a 100-year floodplain. Although there are other candidates this is our preferred estimate for use in cost-benefit analysis of projects that effectively change properties’ floodplain designation. Such findings might have implications for attempts to conduct studies into the impact of other natural hazards. Here too it seems important to control for prior history of study locations.

Our ability to generalise on the basis of these results is hindered by the limited number of studies from outside the US. Out of 37 studies contained in the meta-sample only 5 are from countries other than the US. Hence it is possible that our conclusions are applicable mainly to the US, and the observed floodplain discount is determined by US flood policies. Even inside the US evidence is limited to only 12 States. More research is therefore required for the purposes of understanding better the impact of flood risk on property in other countries.

Apart from the need to conduct studies outside the US other priorities include the following. First it is clear that location within a floodplain is a poor measure of the probability of any individual property flooding. Second, it is necessary to include superior controls for the amenities associated with nearness to the coast e.g. a view of the sea. Finally, it would be interesting to examine the effect on property prices of major engineering projects that in effect change properties’ floodplain designation. A repeat sales study of property prices before the announcement of such works and after their completion would help isolate the benefits of reduced flood risk from other water-related amenity values.



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(references marked with \* indicate studies included in the meta-analysis)

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## APPENDIX (Tables A1 – A3)

**Table A1. Main characteristics of the studies included in the meta-sample**

Study ID <sup>1</sup>	Author	Year	Estimation period	Year last flooded <sup>2</sup>	Flood risk (floodplain)	Average sample	Hedonic specification	Functional form of dependent variable	Econometric model	Notes
1	MacDonald, Murdoch and White	1987	Jan 1985 – Mar 1985	1983	100	139	Standard	Box-Cox	OLS	
2	Skantz and Strickland	1987	Jul 1977 – Jul 1981	1975	100	176	Standard/DND	Semi-log	OLS	
3	Donnelly	1989	Jan 1984 – Dec 1985	1981	100	334	Standard	Linear	OLS	
4	Shilling, Sirmans and Benjamin	1989	Dec 1982 – Feb 1984	1973	100	114	Standard	Semi-log	OLS	
5	Bialszewski and Newsome	1990	1987 – 1989	1983	100	93	Standard	Linear	OLS	
6	MacDonald et al.	1990	Jan 1988 – Jul 1988	1983	100	183	Standard	Box-Cox	OLS	
7	Speyrer and Ragas	1991	1971 – 1986	1969	100	999	Standard	Linear/Semi-log	OLS	
8	US Army Corps of Engineers	1998	Apr 1988 – Mar 1993	1981	100	344	Standard	Linear	OLS	13 flood events during 1974 and 1986, the most destructive in 1981.
9	Bartosova et al.	1999	Jan 1995 – Jul 1998	1986	100 and 500	1,431	Standard	Semi-log	OLS	
10	Harrison, Smersh and Schwartz	2001	1980 – 1997	1964	100	22,411	Standard/DND	Linear	OLS	
11	Shultz and Fridgen	2001	Jan 1995 – Aug 1998	1969	100 and 500	3,783	Standard	Linear	OLS	
12	Troy	2001	Dec 1996 – Jan 2000	1996	100	15,716	Standard/DND	Semi-log	OLS/WLS	Before and after the implementation of the 1998 California Natural Hazard Disclosure Law
13	Dei-Tutu and Bin	2002	Jan 1998 – Jun 2002	1996	100	5,122	Standard	Box-Cox	OLS	
14	Bin	2004	Jul 2000 – Jun 2002	1999	100	1,397	Standard	Semi-log	OLS	
15	Bin and Polasky	2004	Jul 1992 – Jun 2002	1991/1999	100	8,375	Standard/DND	Semi-log	OLS	Before and after Hurricane Floyd in 1999.
16	Troy and Romm	2004	Dec 1996 – Jan 2000	1996	100	21,693	DND	Semi-log	WLS	Before and after the implementation of the 1998 California Natural Hazard Disclosure Law.
17	Hallstrom and Smith	2005	1982 – 2000	1960/1992	100	5,212	DND	Semi-log	OLS	
18	Bin and Kruse	2006	Sept 2000 – Sept 2004	1995	100 and 500	2,895	Standard	Semi-log	OLS	
19	Lamond and Proverbs	2006	2000 – 2005	2000	100	159	Standard	Semi-log	OLS	
20	Daniel et al.	2007	1990 – 2004	1926/1993/1995	100	9,505	Standard/DND	Semi-log	OLS/Spatial	Before and after floods along the Meuse River in 1993 and 1995.
21	Morgan	2007	Jan 2000 – Feb 2006	1998/2004	100	20,882	Standard/DND	Semi-log	OLS	Before and after Hurricane Ivan in 2004. Ivan was the 4 <sup>th</sup> Hurricane to hit Florida in 2004.

(Continued)



Table A1. Continue

Study ID <sup>1</sup>	Author	Year	Estimation period	Year last flooded <sup>2</sup>	Flood risk (floodplain)	Average sample	Hedonic specification	Functional form of dependent variable	Econometric model	Notes
22	Bin et al.	2008a	1995 – 2002	1991	100	990	Standard	Semi-log	OLS/Spatial	
23	Bin et al.	2008b	Sept 2000 – Sept 2004	1999	100 and 500	3,106	Standard	Semi-log	Spatial	
24	Pope	2008	Jan 1995 – Sept 1996	1989	100 and 500	9,349	Standard/DND	Semi-log	OLS	Before and after Residential Property Disclosure Act in 1995.
25	Daniel et al.	2009b	1990 – 2004	1926/1993/1995	100	9,505	Standard/DND	Semi-log	OLS	Before and after floods along the Meuse River in 1993 and 1995.
26	Kousky	2010	1979 – 2006	1973/1993	100 and 500	291,831	Standard/DND	Semi-log	OLS	Before and after flood in 1993 along the Mississippi-Missouri River.
27	Samarasinghe and Sharp	2010	2006	2001	100	2,241	DND	Semi-log	OLS/Spatial	Before and after publication of maps outlining flood-hazard boundaries.
28	Posey and Rogers	2010	2000	1997	100	69,022	Standard	Semi-log	OLS/Spatial	
29	Atreya and Ferreira	2011	1985 – 2010	1959/1994	100 and 500	15,650	Standard/DND	Semi-log	OLS	Before and after Tropical Storm Alberto in 1994.
30	Rambaldi et al.	2012	1970 – 2010	1931	100	3,944	Standard	Semi-log	Spatial	
31	Atreya and Ferreira	2012c	1985 – 2010	1959/1994	100	3,005	DND	Semi-log	OLS/Spatial	Before and after Tropical Storm Alberto in 1994.
32	Atreya and Ferreira	2012a	1985 – 2010	1959/1994	100 and 500	9,958	DND	Semi-log	OLS/Spatial	Before and after Tropical Storm Alberto in 1994.
33	Atreya, Ferreira and Kriesel	2012	1985 – 2010	1959/1994	100 and 500	10,348	Standard/DND	Semi-log	OLS/Spatial	Before and after Tropical Storm Alberto in 1994.
34	Atreya, Ferreira and Kriesel	2013	1985 – 2004	1959/1994	100 and 500	8,042	DND	Semi-log	Spatial	Before and after Tropical Storm Alberto in 1994.
35	Bin and Landry	2013	1992 – 2008	1992/1996/1999	100 and 500	4,080	Standard/DND	Semi-log	Spatial	Before and after Hurricane Fran 1996 and Floyd 1999.
36	Meldrum	2013	1995 – 2010	1974	100	25,512	Standard	Semi-log	OLS/Spatial	
37	Turnbull, Zahirovic and Mothorpe	2013	1984 – 2005	1983	100 and 500	22,351	Standard	Semi-log	OLS/Spatial	

Notes: <sup>1</sup> Corresponds to the same ID as in table 2.<sup>2</sup> For studies using a DND approach before and after a flood the dates correspond to the year of the previous flood for the pre-flood and post-flood sample.

**Table A2. Meta-regression results: Standard hedonic and DID estimates**

Variables	Sample size weights				Characteristics of the study				
	(1) Standard Hedonic	(2) DID estimates	(3) DID before	(4) DID after	published	-0.0258 (0.0278)	-0.00520 (0.0181)	-0.0272* (0.0151)	0.00778 (0.0165)
<i>Flood risk perception</i>					med_sampleyear	0.00364 (0.00333)	0.00332 (0.00246)	-0.0140*** (0.00513)	0.0294 (0.0235)
Mnths	0.830** (0.342)	0.787*** (0.248)	1.079 (0.683)	-1.622 (1.355)	time_span	0.00493** (0.00221)	0.00391 (0.00272)	-0.00614 (0.00777)	-0.0496 (0.0318)
<i>Flood risk level</i>					<i>Regional fixed effects<sup>1</sup></i>				
Risk level	-2.770*** (0.979)	-3.934*** (0.711)	-2.519** (1.217)	-5.292*** (0.656)	louisiana	-0.108 (0.0721)	-	-	-
<i>Context of the study</i>					n_carolina	-0.0673 (0.0543)	0.139 (0.0888)	0.128 (0.177)	-0.359 (0.229)
lav_feet	0.164*** (0.0343)	0.0648 (0.193)	-0.388 (0.477)	0.185 (0.153)	texas	0.135* (0.0692)	-0.0889 (0.360)	0.335 (0.759)	0.920 (0.907)
lavprice_2010	-0.0367 (0.0675)	-0.108 (0.257)	0.305 (0.540)	-0.0300 (0.137)	wisconsin	0.156*** (0.0458)	-	-	
Flooded	0.0217 (0.0306)	-0.00592 (0.0776)	-	-0.0114 (0.0647)	alabama	0.274*** (0.0470)	-	-	
scnd_flood	-	-0.0835** (0.0383)	-0.0667 (0.0560)	0.0130 (0.0155)	florida	0.179*** (0.0559)	0.209 (0.133)	0.479 (0.294)	0.526* (0.290)
dd_after	-	-0.0622* (0.0341)	-	-	california	0.0664 (0.0606)	0.219 (0.196)	0.0453 (0.403)	-0.340 (0.260)
dd_afterlaw	-	0.0473* (0.0256)	-	0.0514* (0.0281)	missouri	0.00675 (0.0585)	0.102 (0.0835)	0.0741 (0.185)	0.389* (0.216)
coast	0.133*** (0.0408)	-	-	-	colorado	-0.0512 (0.0645)	-	-	
<i>Control variables of study</i>					minnesota	0.194*** (0.0505)	-	-	
amenity	0.00367 (0.0139)	-0.250 (0.243)	0.177 (0.536)	0.782 (0.705)	nl	-0.118* (0.0690)	0.252 (0.193)	0.0446 (0.408)	-0.0280 (0.108)
real_p	-0.0718 (0.0448)	-0.0427 (0.105)	0.232 (0.232)	0.148 (0.160)	uk	-0.0640 (0.0729)	-	-	
<i>Characteristics of econometric model</i>					aus	-0.294** (0.138)	-	-	
linear	-0.224*** (0.0463)	-0.408 (0.263)	-0.000767 (0.529)	0.734 (0.736)	nz	-	0.199 (0.358)	-0.163 (0.744)	-0.894 (0.612)
Box-Cox	0.0291 (0.0393)	-	-	-	Constant	-8.823 (6.466)	-6.419 (5.452)	25.80** (10.98)	-58.48 (47.48)
spatial	-0.0113 (0.00855)	0.0108 (0.0263)	-0.0176 (0.0350)	0.0169 (0.0271)	Observations	133	216	115	101
dd_hpm	-	-	-	-	R <sup>2</sup>	0.769	0.748	0.690	0.916
(Continued...)					Adj. R <sup>2</sup>	0.710	0.718	0.628	0.896
					Rmse	0.0430	0.0521	0.0643	0.0257

Note: <sup>1</sup> The omitted region is Georgia, US.

The dependent variable is the effect size  $T$ . Standard errors in parentheses; for results using sample size weights they correspond to Huber-White robust standard errors. \*, \*\* and \*\*\* means rejection of the null hypothesis at the 10 percent, 5 percent and 1 percent confidence level.

**Table A3. Meta-regression results: Only including sample in Daniel et al. (2009a)**

Variables	Sample size weights			Characteristics of the study			
	(1)	(2)	(3)	published	-0.0221 (0.0161)	-0.0213 (0.0143)	-0.0316*** (0.00765)
	Inc. Coast (no months)	No Coast (no months)	No Coast (inc. months)				
<i>Flood risk perception</i>				med_sampleyear	0.00140 (0.00481)	-0.00407 (0.00249)	-0.00485** (0.00198)
Mnths		-	0.601*** (0.152)	time_span	0.000418 (0.00406)	-0.00435** (0.00203)	-0.00545*** (0.00142)
<i>Flood risk level</i>				<i>Regional fixed effects<sup>1</sup></i>			
Risk level	-0.403 (1.541)	-2.717** (1.174)	-2.283** (0.972)	louisiana	-0.0327 (0.0340)	0.0660 (0.0489)	0.00432 (0.0494)
<i>Context of the study</i>				n_carolina	0.0796 (0.0993)	-0.0130 (0.0555)	0.0553 (0.0569)
lav_feet	-0.0606 (0.0786)	-0.0867** (0.0383)	-0.100** (0.0404)	texas	0.0618 (0.0479)	0.109** (0.0539)	0.0896** (0.0442)
lavprice_2010	-0.0408 (0.0698)	0.0422 (0.0267)	0.0448* (0.0267)	wisconsin	-	-0.139*** (0.0135)	-0.137*** (0.0116)
Flooded	-	-	-	alabama	0.135*** (0.0371)	-	-
scnd_flood	-	-	-	florida	0.123* (0.0700)	0.0450** (0.0207)	0.0488*** (0.0158)
dd_after	-0.0734* (0.0429)	-0.0241** (0.00915)	-0.0488*** (0.0113)	california	0.0868 (0.114)	-0.0180 (0.0506)	0.0645 (0.0505)
dd_afterlaw	0.0650* (0.0373)	0.0103** (0.00420)	0.0418*** (0.00742)	missouri	-	-	-
coast	0.0947 (0.0730)	-	-	colorado	-	-	-
<i>Control variables of study</i>				minnesota	0.135 (0.0939)	-0.181* (0.0910)	-0.0531 (0.0971)
amenity	-0.0444 (0.0280)	0.166** (0.0815)	0.0601 (0.0888)	nl	-	-	-
real_p	-0.0307 (0.0403)	0.0226 (0.0223)	0.0760*** (0.0175)	uk	-	-	-
<i>Characteristics of econometric model</i>				aus	-	-	-
linear	-0.129*** (0.0373)	0.131** (0.0570)	0.0910 (0.0553)	nz	-	-	-
Box-Cox	-0.0485* (0.0264)	-0.0454** (0.0170)	-0.0625*** (0.00834)	Constant	-1.854 (9.993)	8.164 (5.022)	9.237** (4.044)
spatial	-0.164* (0.0904)	-	-	Observations	104	77	77
dd_hpm	0.00952 (0.0109)	-0.00364 (0.00418)	-0.00420 (0.00341)	R <sup>2</sup>	0.411	0.736	0.813
(Continued...)				Adj. R <sup>2</sup>	0.241	0.635	0.736
				Rmse	0.0589	0.0204	0.0173

Note: <sup>1</sup> The omitted region is Georgia, US.

The dependent variable is the effect size  $T$ . Standard errors in parentheses; for results using sample size weights they correspond to Huber-White robust standard errors. \*, \*\* and \*\*\* means rejection of the null hypothesis at the 10 percent, 5 percent and 1 percent confidence level.

